

## Exhibit 9B:

Dr. Kelly Socia Supplemental  
Declaration Rebutting Research Cited by  
Defendants

**SUPPLEMENTAL DECLARATION OF KELLY M. SOCIA, PH.D.**

**Rebutting Research Cited by Defendants in  
Response to Plaintiffs' Motion for Preliminary Injunction**

**Prepared by:**

Kelly M. Socia, Ph.D.  
Associate Professor  
School of Criminology and Justice Studies  
University of Massachusetts Lowell  
Kelly\_Socia@uml.edu

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Note: The analyses and views represented in this document are those of Kelly M. Socia, Ph.D., and do not necessarily reflect those of the University of Massachusetts, Lowell.

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## **Executive Summary**

1. This report chiefly is concerned with the social science research cited by Defendants regarding the efficacy of sex offense registries, and what it tells us about sexual offending and sexual recidivism. Most of these studies are cited on pages 20 to 23 of Defendants' response, particularly regarding Defendants' argument that registries could reduce recidivism. ECF No. 39, PageID.1121–1123.

2. Overall, Defendants cite a variety of studies as evidence of SORN laws' effectiveness. Yet Defendants make a series of logical fallacies in presenting these claims, the chief of which concerns cherry-picking data to support otherwise unsupported claims, and relying on raw data that the studies' authors ruled out because it was not up to the scientific standard of 'statistical significance.'

3. While Defendants try to muddy the waters, the research is clear that registries do not work. Both multi-state studies (Prescott & Rockoff, 2011; Vasquez et al., 2008; Walker et al., 2008) and systematic reviews (Washington State Institute for Public Policy, 2009) find no consistent evidence of the efficacy of registration/notification laws in reducing sex crimes. In other words, there is zero solid evidence that these laws make communities safer.

4. Indeed, almost every single study cited by Defendants as 'evidence' of the registry's benefits, when read in full and in context, concludes that the registry *did not* reduce sex crime rates (both overall and recidivism specifically). Yet one

would not know this based on the selective quotes used by Defendants in their response.

5. In the end, Defendants' claims consist of an assortment of cherry-picked data, taken out of context, in an attempt to justify a policy that simply cannot be justified given the existing body of modern social science research.

**Rebuttal to Research Cited by Defendants in Their  
Response to Plaintiffs' Motion for Preliminary Injunction**

6. In this report, as to each study regarding sex offense registries and policies that Defendants cite in their response to Plaintiffs' Motion for Preliminary Injunction, ECF No. 39, I first quote or summarize Defendants' statements or conclusions about that research. Next, I discuss their statements in terms of whether they are supported or refuted by the research on each topic, as well as relevant details about the individual studies quoted or relied upon by Defendants. At the end, I summarize my overall expert opinion regarding the Defendants' response and their use (or misuse) of the social science research findings. My qualifications are set out in my prior declaration. Socia Report, Complaint, Ex. 6, ECF No. 1-7.

7. Overall, Defendants maintain that one reason why registrants are less likely to reoffend is because registries help prevent recidivism. Defendants submit that because registrants are aware that the public knows they are on the registry, registrants may be more likely to comport their behavior to avoid further legal problems. *Id.* at 47; PageID.1200. These arguments are not supported by the research.

**Langan et al., 2003**

8. Defendants cite Langan (2003) for the proposition that “thirty percent of released child molesters and nearly forty percent of released statutory rapists had previously been arrested for sex offenses” (p. 16). *Id.*, PageID.1169. In doing so, Defendants make two key logical errors in touting the high “prior arrest” rate of the individuals in the Langan (2003) study.

9. First, the 30 to 40% prior arrest rate reflects only certain subgroups of individuals reported in that study – child molesters and statutory rapists released from prison – yet Defendants cite it to suggest a high recidivism rate for registrants in general. In fact, the data in that study applies only to a narrow range of individuals.

10. Second, the *prior sexual arrests* statistic is a red herring because by definition and conventional practice, “recidivism” first requires a prior *conviction*, not a prior arrest. In other words, recidivism rates measure whether a person who has been *previously* convicted then reoffends in the *future*. (While some researchers look at new arrests rather than new convictions in measuring recidivism, the starting point in considering recidivism involves criminal conduct *after* a previous conviction.) The statistic Defendants cite instead looks *backwards* at the proportion of people with prior sex crime *arrests* (including arrests occurring before *any* conviction). This statistic says nothing about the true sexual *recidivism* rate of individuals

*post-conviction*, which requires looking at what happens *after* a conviction (and, these days, being subject to registration).

11. In fact, Langan (2003) specifically notes that of the individuals in the study, just 5.3% (517 of 9,691) were *rearrested* for a new sex crime, and only **3.5% were reconvicted during the 3-year follow up period**, with many of the rearrests (40%) occurring within the first 12 months of release from prison (p. 24 and table 21). Further, even among the two high-risk subgroups highlighted by Defendants (child molesters and statutory rapists), the 3-year sexual rearrest rates were 5.1% for child molesters (**3.5% convicted**), and 5.0% for rapists (**3.6% convicted**) (p. 24, table 22). These figures are consistent with Plaintiffs' assertion that overall people who commit sex offenses reoffend (meaning committing a new sexual offense after a conviction) at *much lower rates* than almost all other categories of offenders (who commit a new offense similar to their past offense).

12. It does not take a statistical expert to see that the actual *recidivism rates* just cited are a far cry from the 30 to 40% *rearrest rate* misleadingly trumpeted by Defendants. This is just the first of many instances of Defendants providing 'cherry-picked' data from a source to suggest a conclusion that is not otherwise supported by the study, which is misleading for readers of Defendants' response.

**Alper and Durose, 2019**

13. Defendants cite Alper's 2019 Department of Justice study as evidence

that individuals convicted of sex crimes “were three times more likely than individuals convicted of non-sexual crimes to be rearrested for rape or sexual assault from 2005 to 2014.” *Id.*, PageID.1169–1170. Although technically correct, Defendants use the comparison of rates to imply that people who commit a sexual offense are more dangerous than other categories of lawbreakers.

14. In short, the “three times more likely to be rearrested” claim is misleading when taken out of context. On the very first page of the study, the authors provide part of the context (for the statistic cited by Defendants) by including the actual sex offense rearrest rates: **7.7% rearrest rate for people with past sex offense versus 2.3% for people with non-sex offenses**, over a nine-year follow-up period. (Alper, 2019, p. p. 1).

15. But as Alper (2019) shows, offenders of every category are more likely to be rearrested for a similar crime: released robbers are more likely to be rearrested for robbery; released drug dealers are more likely to be rearrested for dealing drugs, etc. And as Alper (2019) also makes clear, those released for sexual offenses are *much less* likely to commit a new *similar* offense than all other categories of felons but one (murderers).

16. Within the period studied, using the Alper (2019) study’s *rearrest* rates (as opposed to *reconviction* rates):

- sex offenders’ rearrest rate for a new sex offense was 7.7%;
- robbers’ rearrest rate for a new robbery was 16.8%;



- non-sexual assailants' rearrest rate for a new non-sexual assault was 44.2%;
- drug offenders' rearrest rate for a new drug offense was 60.4%;
- property offenders' rearrest rate for a new property offense was 63.5%;
- public order offenders' rearrest rate for a new public order offense was 70.1%;
- and murderers' rearrest rate for a new murder was 2.7%.

17. Put another way, Alper's 2019 DOJ study found that, for example, (non-sexual) assaultive offenders were rearrested for a new (non-sexual) assaultive crime at *six times* the rate that people with past sex offenses were rearrested for a new sex offense, and drug offenders were rearrested for a new drug crime at *nine times* the rate that people with past sex offenses were rearrested for a new sex crime.

18. These results could not be further from the picture Defendants try to paint that people with past sex offenses pose a clear and present danger forever into the future. What Alper (2019) makes clear is that the sexual *rearrest* rate over nine years was just 7.7% for people with past sex offenses in the study – *far lower* than all other categories of felons but one with regard to rearrest for a new crime (similar to the original crime for which they were arrested in the past). Alper, p. 4, Table 2.

19. Alper (2019) also shows that people who committed a sexual offense have the lowest rearrest rate for *any* post-release *violent* crime of all discrete groups of offenders including murderers; and have a lower rearrest rate for *all* subsequent crimes than every other discrete group of offenders except murderers. DOJ Study, Alper & Durose, Table 2, at p 4.

20. Thus, all that Defendants have shown with this study is that people with prior sex crime convictions are more likely to commit future sex crimes when compared to people with non-sex crime convictions – a truism for all categories of crime. The finding is irrelevant in determining the overall ‘risk’ of sexual recidivism, let alone the *current* risk, for people who will be subject to registration, most of them for life.<sup>5</sup>

21. Next, as to Alper (2019), Defendants seize upon the upper range of the estimates of recidivism offered by Plaintiffs’ experts to suggest that there is a “monumental difference” between the average population risk (1-2%)<sup>6</sup> and the upper range of recidivism (15%) (pp. 17–18). *Id.* PageID.1170–71. Yet the range offered by Plaintiffs’ experts is just that – a range – due to the natural differences in recidivism estimates provided by individual recidivism studies. These studies vary based on location, inclusion criteria (like high-risk and low-risk populations), follow-up time, and a host of other factors. The *range* of estimates for the general population and for people with non-sex-offense convictions who are not subject to registration also

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<sup>5</sup> Additionally, Table 3 (p. 5) of the Alper (2019) report shows that the risk of sexual rearrest dramatically drops the longer such individuals stay recidivism-free in the community. Thus, people with past sex offenses have the highest risk of recidivism among the first year or two from release, and then become continually less likely to recidivate the longer they stay offense-free in the community. This finding undercuts the justification for keeping individuals subject to registry provisions for 10+ years in the name of community safety.

<sup>6</sup> See Hanson Report, ECF No. 1-4 (Ex. 3), PageID.243.

varies based on similar factors.<sup>7</sup> Plaintiffs' experts provide a range of recidivism rates (based on multiple factors) in addition to overall average rates in order to provide as complete a picture as possible.

22. Thus, Defendants' use of the extreme upper end of the range for registrants is, again, cherry-picking based on comparing the high end of the range regarding sexual recidivists against the average or the low end of the range for the general population (or for those with non-sexual offenses) committing first-time sex crimes. This is an 'apples to oranges' comparison that has little bearing on the actual risk of sexual recidivism by people with past sex crime convictions, or on the efficacy of the registry.

23. Defendants also question the recidivism estimates' value, due to their accounting only for only those who are caught (p. 17). *Id.*, PageID.1170. "It does not take a Ph.D. statistical expert to question the value of the data if it only represents who returns to court rather than who actually reoffends" (p. 18). *Id.*, PageID.1171.

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<sup>7</sup> Indeed, based on results of 11 studies, Plaintiffs' expert Karl Hanson reports that in different populations of people with a past non-sexual conviction the range for committing a first sexual offense post-release was .2% to 5.8% after five years, with a median value of 1.3%. He concluded that the best estimate of "the rate of spontaneous sexual offences among individuals with nonsexual convictions was in the range of 1-2% within a 5-year period", which "corresponds to an estimated 20-year (lifetime) rate of 3.8%." Hanson Report, ECF No. 1-4, PageID.241-42. For the general population (which has been less studied), Hanson estimates a 20-year (lifetime) rate of "just below 2% can be said to fairly represent the prevalence of sexual offence by males in the general population." ECF No. 1-4, PageID.243-44.

Defendants implicitly assume that everyone who is rearrested is guilty of the new offense, while the Langan 2003 study reviewed above shows significant disparities between the rearrest rates and reconviction rates.

24. Moreover, while it is certain that *some* people convicted of sex offenses will commit new sex crimes that go undetected (and thus will not be included in the estimated range of rearrests or reconvictions), it is equally true that members of the “general population” (i.e., those without a prior sex crime conviction) will also commit sex crimes that go undetected, and thus will not be included in the 1-2% estimate of risk for the general male population. The *relative* difference is therefore unlikely to change significantly, and to the extent that it does, it is likely to increase the first-offense rate for the *general population* more than the reoffense rate for people with prior sex offenses convictions.<sup>8</sup>

**Peterson, et al., 2017**

25. Having used smoke and mirrors in attempting to show that people with past sex offenses are “risky,” Defendants turn to the argument about the “costs” of

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<sup>8</sup> Indeed, it does not take a ‘Ph.D. statistical expert’ to understand that once a person is convicted of an initial sex crime, his “chances of arrest are almost certainly greatly increased if he commits further offences.” (Soothill et al., 1976, p. 62). If a prior conviction increases the chances of detecting a future sex offense, this again suggests that Michigan should devote more resources to encouraging reporting of sex crimes and ensuring successful conviction of first-time offenders, rather than spending millions of dollars on a registry that has little or no deterrent effect, and may actually increase recidivism.

sexual recidivism. They cite Peterson (2017) to show that “the lifetime cost of rape among U.S. adults is estimated to be more than \$122,000 for each assault” (p. 18). *Id.*, PageID.1171. This argument combines two logical fallacies: an appeal to emotion, and drawing unsupportable conclusions from the research.

26. First, Defendants are using the cost estimate as a roundabout way to argue that “even if one victim is saved” it would justify their policies and practices relating to the registry. This is suggested by their follow-up paragraph (p. 18), “for anyone that has been the victim of sexual assault or knows someone that has been the victim of sexual assault, every percentage increase means another lifelong burden carried by survivors.” *Id.*, PageID.1171.

27. This logical fallacy is called an “appeal to emotion”, and is essentially the “even if one life is saved” argument, which has a *long* history of use in justifying ineffective criminal justice policymaking (see Socia & Brown, 2016). Yet despite its emotional appeal, and its value as a political tool, such an argument has zero bearing on whether any given policy is effective or rational.

28. Sexual offenses unquestionably can impose severe harm on victims. But just because a policy is *intended* to address a significant harm does not mean the policy is *effective* at reducing that harm. Registries are ineffective in preventing the harm caused by sexual offending because they do not actually reduce sexual offending.

29. A particular danger of the emotional “one life saved” argument is that it is used to justify imposing other harms – harms to registrants – without any proof that such harms have any impact on reducing the harms of sexual offending. Additionally, implementing ineffective policies like the registry can lead to a false sense of security for members of the public (Zevitz, 2006), which could in turn increase the risk of victimization by individuals *not* on the registry, which comprise the vast majority of sexual offenses. (Sandler, 2008)

30. Second, in citing Peterson (2017), Defendants draw unsupportable conclusions from that research. Defendants may counter that looking critically at the research “discounts” the estimated cost of a sexual assault and is insensitive towards the suffering of victims. But like all research, research on the costs of sexual offending must be examined carefully.

31. As an initial matter, this study looks at the costs of rape, which was “defined as any lifetime completed or attempted forced penetration or alcohol- or drug-facilitated penetration, measured among adults not currently institutionalized.” (Peterson, 2017, p. 691.) People on registries have been convicted of a wide range of offenses, which can range from sexual activity with willing underage partners, to non-contact offenses like viewing images, to rape. Not all sex offenses impose the same costs.

32. With respect to the ‘costs’ of rape, the Peterson study uses *extremely*

broad criteria to generate the \$122,000-per-victim estimate of such costs, including, for example:

- Lifetime “estimated lost work productivity and medical payments for asthma and joint pain” (Table 1 & p. 695);
- Lifetime “estimated medical payments and lost productivity attributable to cancer” (Table 1 & p. 695);
- Lifetime “lost work productivity and medical costs for excess alcohol use and smoking” (Table 1 & p. 695); and
- Lifetime costs associated with an eating disorder (Table 1).

The amount also includes the lifetime costs of lost productivity *of the perpetrator* due to incarceration (Table 1). This is by no means a comprehensive list of the nebulous ‘lifetime cost’ categories used by the authors, but these serve as examples of the overly inclusive criteria used to estimate the per-victim costs. Indeed, ‘lost work productivity’ related costs alone accounted for 52% of the per-victim cost estimate. (Peterson 2017, p. 697.)

33. There are other methodological decisions in this study that should be considered as well. For example, to determine lifetime cost estimates, the authors assume *all* victims to be 18 years old at the time of the assault, and that they *all* live to be 79 years old (the average life expectancy), thus accumulating *61 years’* worth of lifetime costs potentially attributable to the sexual assault. Indeed, when considering just the ‘acute’ outcome costs (i.e., more immediate costs, such as property loss/damage, injuries, fatalities, pregnancy, and immediate lost productivity), the

estimate is closer to \$2,000 per victim. Thus, it is the laundry-list of long-term/life-time outcomes, which include the broad inclusions listed earlier, that account for the remaining \$120,000+ of the estimate. (Peterson 2017, Table 1.)

### **Statistics Only Represent Male Offenders**

34. Defendants note that “many of the statistics relied upon by the experts, only represent adult male offenders”, while “female offenders make up a surprising percentage of offenders” (p. 19). *Id.*, PageID.1172. Defendants cite Stemple (2014) as evidence, who notes that men and women had “similar prevalence of nonconsensual sex.”

35. Yet the statistic cited in Stemple (2014) refers only to sexual *victimization* – it says nothing about sexual *recidivism*, whether of male or female offenders. Thus, it has no bearing on the *recidivism* risk posed by female offenders who, by definition, have been previously arrested and convicted of a sex crime, and released back into the community to therefore be ‘at risk’ for sexual recidivism.

36. Indeed, Plaintiffs’ expert report by Ulrich (ECF No. 1-9, PageID.611) addresses the female *recidivism* question with the following statements:

The base rate of sexual recidivism among women is very low, and given the absence of a recidivism prediction tool, the base rate is the best available metric for assessing risk and informing social and legal policies. (*Id.*, PageID.612)

[T]he base rate for female sexual recidivism is between 1% and 3% at the time of release. (*Id.*)



So, women who are not chronic offenders and have been living offense-free in the community for more than a few years have a very low risk of reoffending. (*Id.*)

37. Other sources note both that the proportion of individuals arrested for sexual offenses who are women is remarkably low, and that the sexual recidivism risk of female offenders is much lower than the rate of male offenders:

Female offenders accounted for ~1% of those arrested for rape in 2000. (Sandler, 2007.)

Female offenders make up only 2% of registered sex offenders in New York State. (Sandler, 2007.)

[It should be stressed that even if reliable and valid sexual recidivism risk markers are identified for female sex offenders, those female sex offenders deemed to be at high risk of sexual recidivism relative to other female sex offenders **would still be at relatively low risk compared with similarly situated male sex offenders.** (Sandler 2009, p. 468.)

38. Thus, despite Defendants' claims regarding a 'surprising percentage' of offenders who are female, this claim is taken out of context, and has no bearing on sexual recidivism risk, nor on the proportion of *registrants* who are female. That is, not only do women account for an exceptionally small percentage of individuals convicted for sex crimes (and relatedly, registrants), but their risk of sexual recidivism is also exceptionally low. Given these low base rates, such research only seems to support Plaintiffs' claims about the ineffectiveness of the registry in protecting the public from sex crimes.

**Schram and Milloy, 1995**

39. Defendants note that “Facts incorporated by reference in Plaintiffs’ complaint also supports the connection between SORA and law enforcement’s increased capacity to apprehend recidivist sex offenders” (p. 19). *Id.*, PageID.1172. Defendants cite Schram and Milloy (1995) for the proposition that SORN laws can increase the capacity to apprehend recidivist sex offenders (p. 19). *Id.*, PageID. 1172.<sup>9</sup>

40. First, it is important to note that the data used in the Schram (1995) study is almost 30 years old, when sex offense registries were brand new, and had few of the features of today’s third-generation super-registries. Second, the study focuses on “the most serious sex offenders, those subject to a Level III notification.” (Schram & Milloy, 1995, p. 5) Third, the authors found law enforcement to be selective and “judicious in their use of Level III community notification.” (Schram 1995, pp. 19–20.) In other words, the group subjected to the highest level of notification (and thus considered in the study) were apparently carefully selected by law enforcement due to their perceived high risk.

41. This is in striking contrast to the one-size-fits-all approach of Michigan’s SORA 2021, which ties the tier assignment simply to the crime of conviction,

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<sup>9</sup> In my report I use the acronyms SOR, SORN, and SORA interchangeably based on the specific study or law being referred to. These all generally refer to the laws implementing registration and/or notification provisions.

which today we know does not correlate with risk. Thus, this study is certainly not a comprehensive analysis of a general group of individuals who might be similar to those subject to the Michigan registry, and instead is biased towards the highest risk individuals who were carefully selected for the Level III notification provisions.

42. Yet even accepting this study's high-risk focus, the authors note that the community notification provision – which was a key rationale for most registries then and now – had *no statistical impact on sexual recidivism*. Indeed, when discussing the lack of significant differences in sexual recidivism between the notification and non-notification groups, the authors note that **“This finding suggests that community notification had little effect on sexual recidivism as measured by official reports of new arrests.”** (Schram 1995, p. 17.) Defendants simply ignore this finding.

43. Instead, Defendants focus on the elapsed time from release to rearrest for any new crime (not specific to sex crimes). That is, Defendants appear to cite this study as evidence of SORA's effectiveness because individuals subject to the highest level of notification were *rearrested sooner post-release for any crime* (compared to a cohort who were not subject to such notification because they were released before the new law took effect).

44. Yet this small decrease in time to rearrest was found only for rearrests for *any new offenses, not for follow-up sexual crimes*. Indeed, when comparing the

time to rearrest for *sexual crimes*, the authors note that “the estimated rates of arrests for sex offenses are remarkably similar for each group [notification and non-notification] throughout the follow-up period.” (Schram 1995, p. 17; *see also* Figure 1.)

45. Thus, Defendants again cite one largely inconsequential finding (that notification may slightly decrease the period from release to rearrest for *any new crimes*). They then use this as broad evidence of SORA’s ‘increased capacity to apprehend recidivist sex offenders, while conveniently ignoring the other, arguably more important findings that the notification law affected *neither* sexual recidivism rates *nor* the length of time to rearrest for new sexual crimes.

46. The authors themselves even note the problems with relying on the time-to-arrest finding: “This finding is difficult to interpret without a qualitative examination of changes in law enforcement and community behavior as a result of the community notification law. Such an examination might ask if sex offenders who are subjects of Level III notifications are watched more closely after the law, and whether this increased attention results in earlier detection of criminal behavior,” rather than the new law itself. (Schram 1995, p. 19.)

47. In other words, it is not clear whether the slightly shorter time to rearrest (for *any new crime*) was caused by the notification provision itself, or because law enforcement were simply paying more attention to specifically identified high-risk individuals following the law’s passage.

48. Indeed, an alternative explanation for the increased speed to rearrest for any new crime may be that the notification provisions made it more difficult for such individuals to successfully reintegrate into the community after release. If the notification provisions led to increased burdens on successful reentry, then it is not surprising that individuals subject to this intense supervision might be rearrested sooner (for any new crime) than those without such burdens.

49. The takeaway from the authors remains that the community notification provision had *no effect* on the sexual recidivism rate. That is, they found no evidence that sex crimes were prevented. This is not clear in the way Defendants present the study, but the study itself could not be clearer.

50. Defendants also cite Schram (1995) to note that “22% of unregistered Washington sex offenders were re-arrested for a sex crime within four years, while only 19% of registered sex offenders were re-arrested in the same time frame” (p. 20). PageID.1173. The Schram (1995) study *does* note that “At the end of 54 months at risk, the notification group had a slightly lower estimated rate of sexual recidivism (19%) than the comparison group (22%)” (Schram 1995, p. 3.) If one were to stop reading there, it would seem to affirm Defendants’ claim.

51. But the sentence *that immediately follows*, uncited by Defendants, says: **“This difference was not found to be statistically significant.”** (Schram, 1995, p. 3.) The takeaway is that the community notification provision considered by the

study, and applied as noted earlier to a select group of the highest risk individuals, did not affect sexual recidivism to a degree that could be deemed ‘statistically significant.’

**Zevitz, 2006 (used as a citation for Schram and Milloy, 1995)**

52. Defendants then cite Zevitz (2006) after stating that “Offenders who were subjects of community notification were arrested for new crimes much more quickly than comparable offenders who were released without notification” (p. 20). *Id.*, PageID.1173. In their citation, Defendants refer to figure 1 of Zevitz (2006).

53. However, with this citation, Defendants are erroneously citing Zevitz’s (2006) own summary of the Schram (1995) findings in Washington State. (That is, Defendants seemingly cite Zevitz (2006) *as if it refers to Zevitz’s (2006) own Wisconsin sample*, when it actually is summarizing Schram’s (1995) Washington study.)

54. The problems with this selective quoting of the Schram (1995) findings are set forth above. Yet Zevitz (2006) provides further information about the Schram (1995) findings that are not noted by Defendants: “Although Washington State’s notification group had a slightly lower rate of sexual rearrest than that state’s comparison group, the pace of those rearrests was essentially the same. There was no delay found in the onset of sexual rearrests with the notification group.” (Zevitz,

2006, p. 202.) In other words, Schram (1995) had a non-finding, with rates of sexual rearrest that were ‘essentially the same’.

**Zevitz, 2006 (own study)**

55. It is helpful to look into the findings of Zevitz’s (2006) own research that Defendants seemingly either ignored or grossly misinterpreted with the earlier citation. Zevitz’s 2006 study considered all male individuals, released from Wisconsin prisons between September 1997 and July 1999 for sex crimes, whom the state “ruled eligible for the highest level of community notification.” Thus, it is a sample of high-risk individuals who were *all* “closely monitored and highly restricted in their everyday behavior,” (p. 199) and “in varying stages of sex offender treatment.” (Zevitz, 2006, p. 199.)

56. Zevitz (2006) provides a clear summary of the study, and its conclusion: “A comparison of the recidivism rates for the extensive notification and limited disclosure groups of research subjects addresses the central issue of this study, namely, **whether or not community notification makes a difference. Clearly, it has not.**” (Zevitz, 2006, p. 200.) In other words, community notification in the SORN law studied *did not* affect recidivism rates.

57. Zevitz makes the point a few pages later: “The findings of this exploratory study suggest that extensive amounts of public exposure for sex offenders

released from prison has had little effect on their recidivism as measured by their return to prison.” (Zevitz, 2006, p. 204.)

58. Zevitz (2006) also addresses the ‘time to failure’ issue (which is one of the arguments Defendants’ make regarding the Schram (1995) study as evidence of community notification’s effectiveness.) In this case, Zevitz (2006) compares the ‘extensive community notification’ group with the ‘limited notification group’ of individuals, concluding that the results “stand in contrast to those of Washington State.” (Zevitz, 2006, p. 201.)

59. Specifically, after noting that the findings suggest no effect of notification on recidivism rates, Zevitz (2006) then notes, “Nor do the findings suggest that alerting the community to [registrants’] presence significantly shortened the amount of time before recommitment for those offenders who did recidivate.” (Zevitz, 2006, p. 204.) In other words, in Wisconsin, extensive community notification had no effect on either recidivism (sexual or otherwise), or on time to recommitment.

60. Indeed, recall that Defendants cited figure 1 in Zevitz (2006), apparently as evidence that “Offenders who were subjects of community notification were arrested for new crimes much more quickly than comparable offenders who were released without notification” (p. 20). *Id.*, PageID.1173. Yet Zevitz (2006) notes on page 201: “The difference between the failure times for the extensive notification and comparison groups was insignificant **as Figure 1 indicates.**”



61. Two other points may be useful to consider from this study. First, in Wisconsin, “state correctional personnel conduct a risk assessment on all sex offenders before they are released from prison in generating a pool of candidates which local and county law enforcement administrators use in deciding who should undergo extensive notification.” (Zevitz, 2006, p. 196.) This is similar to the recommendations made by Plaintiffs’ experts regarding individualized risk assessments.

62. Second, “even though almost half of the sex offenders studied in both states [Wisconsin and Washington] recidivated during the 54 months following release from incarceration, most of the recidivism that occurred was for *nonsexual* crime or, particularly in Wisconsin, for technical violation of their probation/parole.” (Zevitz, 2006, p. 204.) Thus, it seems much of the risk of “recidivism” comes *not* from committing *new* crimes (sexual or otherwise), but instead for simply violating the (extensive) conditions of release.

63. In the concluding section, Zevitz (2006) presents a very clear summary of the overall takeaway of the study: “[T]he findings presented here provide negligible support for sex offender community notification having any kind of measurably deterrent effect on sex offender recidivism patterns. If anything, these findings call into question the utility of this practice and **the danger of creating a false sense of security** in the communities where notification occurs.” (Zevitz, 2006, p. 205.)

**Adkins et al., 2000**

64. Defendants cite Adkins' (2000) study of the Iowa registry to suggest that registries work, noting that 3.5% of *unregistered* offenders were reconvicted of another sex crime, while only 3% of *registrants* were reconvicted, within 4 years. *Id.*, PageID.1173–1174.

65. In the authors' own words, however, "The differences in recidivism were not found to be statistically significant." (Adkins et al, 2000, p. 10.) In short, the study *does not support the proposition* for which Defendants cited it.

66. Further, the authors note that the pre- and post-registry groups differ in their average risk scores for recidivism, thus making comparisons even more questionable. Specifically, the pre-registry group was inherently higher risk than the registry group (8.4 average risk assessment score vs. 7.1 average risk assessment score, respectively). As the authors note: "Based on the total risk scores, offenders in the registry sample appeared somewhat less likely than offenders in the pre-registry sample to recidivate." (Adkins et al., 2000, p. 9.)

67. This hypothesis is later confirmed by the authors, who note that "Using the community-based risk assessment as a controlling factor, the differences found between the study groups were consistent with the probability of recidivism identified by the assessment scores." (Adkins et al., 2000, p. 10; see also Table D, Appendix II.) As final confirmation, the authors conclude: "The results found in this

study suggest that the registry had mixed effects on recidivism, but the findings were not statistically significant and could have occurred by chance.” (Adkins et al., 2000, p. 21.)

**Barnoski, 2005**

68. Defendants cite Barnoski’s (2005) study of individuals convicted of sex crimes in Washington as evidence that the registry led to lower felony sex crime recidivism. Specifically, they state that “Unregistered sex offenders released from prison in Washington from 1986 to 1999 were convicted for a felony sex crime at a rate of 7%, registered sex offenders released from prison after Washington’s sex offender registry was enacted were convicted for a felony sex crime at a rate of 4%, while sex offenders convicted after Washington increased its reporting requirements in 1997 were convicted of a felony sex crime at a rate of 2%” (p. 21). *Id.*, PageID.1174.

69. Yet Barnoski (2005) notes on page 1 that while “[v]iolent and sexual felony recidivism by sex offenders in Washington has decreased since passage of the 1997 statute, **the causal link to notification laws is not proven by this research.**”

70. Barnoski (2005) provides alternative explanations that undercut Defendants’ claims that the registry was responsible: “Other conditions may be contributing to this reduction, such as the national and state drop in crime rates and the

state's increased incarceration (incapacitation) of sex offenders.” (Barnoski, 2005, p. 1.)

71. In a bit of insurance for these claims, Barnoski (2005) then follows up with, “[T]he drop in recidivism rates by sex offenders is clear, and the influence of community notification laws cannot be ruled out.” (Barnoski, 2005, p. 1.) In other words, recidivism rates were lower in later cohorts (which is unsurprising, as most crime rates decreased during the late 80's to late 90's), but Barnoski's (2005) study cannot be used as conclusive evidence of the registry and/or community notification laws being responsible for the drop – he cannot say either way.

72. Furthermore, Sandler (2008) provide an excellent summary of the many problems inherent in both Barnoski's (2005) methods, as well as the extraneous influences on his results:

(a) Barnoski's analytic technique did not account for historical crime trends, and (b) as Barnoski's regressions were performed on autocorrelated data, the coefficient standard errors were likely deflated and, therefore, appeared more significant than they in fact were. The possibility of a natural drop in the crime rate or some non-sex offender related factor contributing to Barnoski's findings is supported by the fact that Washington State's rate of violent crimes (per 1,000 population) dropped each year from 1995 to 2006, while its rate of property crimes (per 1,000 population) dropped each year from 1995 to 2003 (Washington Statistical Analysis Center, 2008). **Thus, it appears likely that the reductions in the sexual and violent felony recidivism of sex offenders observed by Barnoski may have been at least in part due to these trends, and once these trends were controlled for in the present study, the impact of registration and notification laws failed to reach significance.** (Sandler et al., 2008, pp. 295–296.)

73. Duwe and Donnay (2008) also provide important critiques of Barnoski's (2005) study in a more concise manner: "Little confidence can be placed in these findings, however, because Barnoski did not use a quasi-experimental design that compared recidivism between a group of offenders who were subject to community notification with a similar group of offenders who were not." (Duwe and Donnay, 2008, pp. 416–417.)

**Vasquez et al., 2008**

74. Defendants cite Vasquez and colleagues (2008) as evidence that the "Number of rapes reported to police decreased in three States following their implementations of sex offender registries" (p. 21). *Id.*, PageID.1175.

75. First, Vasquez (2008) analyzed a total of 10 states. Their own abstract notes the mixed findings: "The results of the analyses are mixed on whether the enforcement of sex offender registration had a statistically significant effect on the number of rapes reported at the state level. Although several states showed a non-significant increase in the number of rapes, only three states had a significant reduction in rapes." (Vasquez et al., 2008, p. 175)

76. Other quotes from this same study provide further evidence that the results are not what Defendants suggest:

Although possible explanations for these results are discussed in the next section, **the evidence does not offer a clear or unidirectional conclusion as to whether sex offender notification laws reduce rapes.** (Vasquez 2008, p. 187.)

The empirical finding of this research is that the **sex offender legislation seems to have had no uniform and observable influence on the number of rapes reported in the states analyzed.** . . . Taken collectively, the findings reported here indicate that sex offender registration and notification laws may have had little general deterrent effects on the incidence of rape offenses analyzed.” (Vasquez 2008, p. 188.)

### **Sandler et al. 2008**

77. Defendants cite Sandler et al. (2008), stating that “Monthly arrests of recidivists in New York for sex offenses generally, and for rape and child molestation in particular, decreased after the State’s sex offender registration act was implemented” (p. 21). *Id.*, PageID.1174–1175.

78. But Sandler (2008) clearly contradict Defendants’ interpretation of it.

The study states:

[N]one of the intervention coefficients for any of the nine types of sexual offending reached significance and were, therefore, all statistically no different than zero. (Sandler 2008, p. 295.)

Thus, it appears that the enactment of SORA had little, if any, impact on rates of general offending in New York State and no significant impact on rates of sexual offending. (Sandler 2008, p. 296.)

The question of how society can best be protected from sexual victimization remains, but empirical research, in both previous studies and the current one, indicates that existing registration and community notification laws are largely ineffective. (Sandler 2008, p. 300.)

### **Duwe and Donnay, 2008**

79. Defendants cite Duwe (2008) to claim that “Unregistered Minnesota sex offenders recidivated at higher rates than registered sex offenders” (p. 22). *Id.*,

PageID.1175. This is actually one of the few studies that finds *some* evidence that community notification *may* deter sexual recidivism. But this study has been criticized for a number of methodological errors or faults.

80. First of these is the differences between the groups being compared in the study, which the authors themselves note: “[S]ubstantial differences existed between the two groups [pre-notification and notification] for variables like ISR [intensive supervised release], prior sex crimes, length of stay, and supervision length.” (Duwe 2008, p. 422, n. 7.) Further, the pre-notification group had less treatment, shorter and less intense supervision, and fewer supervised release revocations.

81. Additionally, while Duwe (2008) finds that the pre-notification and non-notification groups did have higher sexual recidivism rates than the notification group, they *also* had higher nonsexual recidivism rates, which suggests the comparison groups were generally riskier than the notification group. (Duwe 2008, p. 429, Table 4.)

82. Duwe (2008) also notes the special circumstances that the notification group (“level 3 offenders”) were subjected to in Minnesota:

Moreover, nearly every level 3 offender is released to intensive supervision. When sex offenders are placed on ISR, they are continuously supervised by a team of three to five supervision agents, whose case-loads are capped at 15 per state law. During all four phases of ISR, offenders are required to maintain steady employment and to comply with random alcohol/drug testing, and they are subjected to unannounced face-to-face contacts with their supervision agents at both their residence and place of work. (Duwe 2008, p. 441.)

83. Finally, Duwe (2008) notes that community notification is not as simple and effective as Defendants' selective quote suggests: "Community notification is, therefore, a **double-edged sword**. Although it seems to decrease sexual recidivism, it also creates numerous adverse collateral consequences for sex offenders, which makes it difficult for them to re-enter the community successfully." (Duwe 2008, p. 443.) Defendants mention none of these limitations.

**Zgoba et al. 2008**

84. Defendants cite Zgoba (2008) for the proposition that "Unregistered New Jersey sex offenders recidivated at higher rates than registered sex offenders" (p. 22). *Id.*, PageID.1175.

85. But Zgoba (2008) itself summarizes its findings as follows (at p. 2):

Megan's Law has **no effect** on community tenure (i.e., time to first re-arrest).

Megan's Law showed **no demonstrable effect** in reducing sexual re-offenses.

Megan's Law has **no effect** on the type of sexual re-offense or first-time sexual offense (still largely child molestation/incest).

Megan's Law has **no effect** on reducing the number of victims involved in sexual offenses.

86. Zgoba (2008) also notes (at p. 2) that:

Costs associated with the initial implementation [of Megan's law] as well as ongoing expenditures continue to grow over time. Start-up costs



totaled \$555,565 and [2007] costs . . . totaled approximately 3.9 million dollars for the responding counties.

Given the lack of demonstrated effect of Megan’s Law on sexual offenses, the growing costs may not be justifiable.

87. In short, contrary to what Defendants suggest, Zgoba (2008) found that Megan’s Law had no significant effects on sexual recidivism in New Jersey, and further, the authors questioned the wisdom of implementing a law that was expensive to operate for no public safety benefit.

**Letourneau et al. 2010**

88. Defendants cite Letourneau et al. (2010) saying that: “From a sample, 9.2% of unregistered South Carolina sex offenders were charged with a new sex crime over 8.4 years, while only 7.1% of registered sex offenders had a new sex crime charge over the same period” (p. 22). *Id.*, PageID.1175.

89. Contradicting Defendants’ claim involves no more effort than reading the abstract: “**Registration status did not predict recidivism in any model.** These results cast doubt on the effectiveness of broad SORN policies in preventing repeat sexual assault.” (Letourneau 2010, p. 435.)

90. The authors’ discussion provides more context about this conclusion, in case there was any doubt about the findings:

**Results indicated that offender registration status at the time of recidivism was not associated with reduced risk of sex crime recidivism or reduced time to detection of sex crime recidivism.** Consistent results were obtained whether recidivism was defined as new

charges or new convictions and whether models examined sex crime recidivism alone or in the context of competing risks models with other types of recidivism events. **There was no evidence that South Carolina’s broad SORN policy decreased sex offender recidivism rates.** (Letourneau 2010, pp. 452–453.)

### **Tewksbury and Jennings, 2010**

91. Defendants cite Tewksbury and Jennings (2010) to claim that: “Pre-SORN sex offenders in Iowa recidivated at a rate of 16.5%, while post-SORN sex offenders recidivated at a rate of 15.8%” (p. 23). *Id.*, PageID.1176.

92. Again, the authors directly refute that the raw data Defendants cite has the statistical significance they claim for it. These quotes are all on page 579:

The results of this study suggest that SORN has not reduced the rate of sex offender recidivism, nor has it led to a decrease in the number of offenses committed by recidivating sex offenders.

Among a 10-year cohort of Iowa sex offenders, not only is the sexual recidivism rate virtually identical prior to and following the implementation of SORN, but so too is the distribution of sex offenders into trajectory groups essentially identical.

The findings suggest that not only are very few sex offenders likely to sexually recidivate, but the policy also appears to have virtually no impact on sex recidivism.

### **Freeman, 2012**

93. Defendants then cite Freeman (2012) to claim that: “8% of New York sex offenders not subject to community notification recidivated while only 6.3% of sex offenders subject to community notification recidivated” (p. 23). *Id.*, PageID. 1176.

94. This claim, too, runs counter to the author's own conclusion, which is that: "The findings yield implications for sex offender interventions and public policies and **suggest that notification may not be an effective strategy for significantly reducing sexual offenses.**" (Freeman, 2012, p. 539.) "In fact, not only do the results indicate that community notification is not reducing rates of recidivism, but they lend support to the conclusion that **notification increases rearrests.**" (Freeman, 2012, p. 559.)

### **Tewksbury et al., 2012**

95. Defendants cite Tewksbury (2012) to claim that: "13% of pre-SORN sex offenders in New Jersey recidivated while only 9.7% of post-SORN sex offenders recidivated" (p. 23). PgID.1176. This suggests that SORN resulted in statistically significant recidivism decreases.

96. Yet again, Defendants' claim is directly refuted by the authors' own words. The abstract notes that, "**SORN status was not a significant predictor of sex or general recidivism.**" (Tewksbury 2012, p. 308.)

97. The authors provide more detail in the text about this unambiguous conclusion:

In these matched samples of sex offenders released from prison in New Jersey prior to and following the implementation of sex offender registration and notification, **it is clear that there are limited observable benefits of SORN regarding sex recidivism and general recidivism.** With an overall low rate of sex offense recidivism, SORN status failed

to predict which sex offenders would re-offend sexually... Furthermore, **SORN status was not a significant predictor of which sex offenders would re-offend in general, including non-sexual recidivism.** (Tewksbury, 2012, pp. 323–324.)

**Prescott and Rockoff, 2011**

98. Defendants then cite Prescott and Rockoff (2011) to claim that “One of Plaintiffs’ own experts, Professor J.J. Prescott, concluded that ‘requiring registration reduces recidivism’ based on the results of a 2011 study he co-authored” (p. 23). *Id.*, PgID.1176.

99. Yet this four-word quote ignores the key caveat noted by the authors on the page following the quote. Specifically:

. . . any beneficial effect of registration on recidivism is dampened by the use of notification, and it signals that the punitive aspects of notification laws may have perverse consequences. Specifically, a basic trade-off may exist: whereas some nonregistered or potential offenders may be deterred by the threat of notification and its associated costs, the ex post imposition of those sanctions on convicted offenders may make them more likely to recidivate (p. 181).

100. Defendants again cherry-pick a quote, just four words long, that seems to imply that the authors found *only* that the registry reduces recidivism, **while ignoring the finding that SORN laws like Michigan’s, which include *community notification*, may increase recidivism.** See Prescott Report, ECF No. 1-6, PageID. 460–464 (based on updated research, summarizing findings). If Defendants are serious about relying on the Prescott (2011) findings to inform policy decisions, then

Defendants would be well served to remove the public-facing registry and have it continue as a resource solely accessible to law enforcement.

**Non-significance vs. Insignificance**

101. Defendants, perhaps recognizing their misuse of the above studies, say:

Perhaps, Plaintiff's experts will opine that some of the reductions are statistically insignificant, thus are not technically considered reductions in recidivism. However, there are real people behind the numbers – moms, sisters, daughters, dads, brothers, and sons – that do not have to bear, what is often, a lifetime of recovery from being sexually assaulted. So, if recidivism is reduced by one percent, perhaps that means 4,000 or 400 less people are victims of sexual assault – those that are saved from victimization would say that the one percent reduction is significant (p. 4). *Id.*, PageID.1177

102. Defendants misunderstand the meaning of 'statistical significance.' A finding in the data that is deemed 'statistically insignificant' is *not* equivalent to being 'insignificant' in common parlance, nor does 'statistical significance' tell us anything about the *substantive* significance of the data. In layman's terms, 'statistical significance' is used to determine whether a given finding is due simply to chance, as opposed to a measurable variable.

103. In virtually all of the studies cited by Defendants, the quoted findings did not reach the level of 'statistical significance' required by scientists to be reliable, and thus could not be attributable to the policy/practice itself (registration, notification, etc.) as opposed to being a *random variation* in the crime rates or other

factors. In other words, one cannot reliably say that the policy or practice was responsible for the change seen in the data (if any).

104. Instead, Defendants dismiss the concept of statistics entirely, and offer the ‘if only one person is saved’ argument in support of SORN law implementation. Thus, having no foundation on which to base their position (that the registry works) supported by data, Defendants choose to fall back on the ‘appeal to emotions’ fallacy, arguing that *any* reduction of recidivism, regardless of the cause, would be considered ‘significant’ by victims.

105. This is a dangerous argument on which to base criminal justice policy, and while these appeals to emotion have a long history of use in justifying (ineffective) criminal justice policymaking (*see* Socia & Brown, 2016), they have no bearing on whether a given policy is effective or rational.

106. The ‘even one victim saved’ argument also backfires on Defendants, given the research showing increases in sex crimes after implementation of new SORN laws (e.g., Vasquez, 2008) in some states, or as a result of SORN laws making successful re-entry more difficult, leading to additional sex crimes (e.g., Prescott Report, ECF No. 1-4).

107. Plaintiffs’ experts rely only on statistically significant data in determining whether a policy/practice *had* any effect beyond random variation, which is standard scientific practice in determining the effectiveness (or not) of a policy. This

does not discount the harm to a given victim or the cost of a crime, but instead is a means to neutrally determine if any changes observed in the data were due to SORN laws (and various related provisions), or instead were merely random fluctuations.

**Are Sexual Recidivism Rates Low *Because of the Registry?***

108. Defendants muse that “perhaps the reason why registrants are less likely to reoffend is because the registry helps prevent recidivism” (p. 47). *Id.*, PageID. 1200. In this statement, Defendants show a lack of awareness of decades of existing research regarding the recidivism rates of people with past sex offenses, which have not changed (in a statistically significant way) before *and* after the implementation of registries.

109. Defendants also posit that perhaps the recidivism rates of people with past sex offenses are lower than the recidivism rates of people with other types of convictions because the registry is somehow working. But again, the studies show the same *consistently* low recidivism rates for sexual crime, regardless of whether these studies were conducted before or after a registry’s existence.

110. Were registries working as Defendants imply, one would expect to see consistently higher sexual recidivism rates among people with sex offense convictions *before* the registry’s enactment, and much lower sexual recidivism rates *after* its enactment. But the scientific consensus shows the same low sexual recidivism rates both before *and* after a registry’s enactment – especially compared to other

categories of crimes. As Letourneau (2011, p. 15) reported: “the results from group comparison studies failed to support the effectiveness of registration and notification policies in reducing sex crime recidivism rates.”<sup>10</sup>

111. In short, Defendants’ position that sexual recidivism rates are low *because* the registry works is not supported by either the research on the effects of the registry itself, nor by the research on sexual recidivism rates in different periods of time (before and after the registry existed).

### **Conclusion**

112. Overall, Defendants cite a variety of studies as evidence of SORN laws’ effectiveness. Yet as has been documented above, Defendants make a series of logical fallacies in presenting these claims, the chief of which concerns cherry-picking data to support otherwise unsupportable claims, and relying on raw data that the studies’ authors ruled out because it was not up to the scientific standard of ‘statistical significance.’

113. While Defendants try to muddy the waters, the research is clear that registries do not work. Both multi-state studies (Prescott & Rockoff, 2011; Vasquez

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<sup>10</sup> Letourneau (2011) notes that there was one exception to this trend, which involved Duwe’s (2008) finding of a significant effect for Minnesota’s notification policy on sex crime recidivism. However, Letourneau (2011, p. 16) also highlights that Duwe (2008) had a “3-year sex crime recidivism rate of nearly 33% for their prenotification group. This short-term recidivism rate is substantially higher than typically reported [Bureau of Justice Statistics, 2003] and suggests the possibility of a selection effect.”



et al., 2008; Walker et al., 2008) and systematic reviews (Washington State Institute for Public Policy, 2009) find no consistent evidence of the efficacy of registration/notification in reducing sex crimes. In other words, there is zero solid evidence that these laws make communities safer.

114. Indeed, almost every single study cited by Defendants as ‘evidence’ of the registry’s benefits, when read in full and in context, conclude that the registry *did not* reduce sex crime rates (both overall and recidivism specifically). Yet one would not know this based on the selective quotes used by Defendants in their response.

115. In the end, Defendants’ claims consist of an assortment of cherry-picked data, taken out of context, in an attempt to justify a policy that simply cannot be justified given the existing body of modern social science research.

### **Statement of Compensation**

I am being compensated at a rate of \$250/hour for the work in researching and writing this report. For court testimony/ deposition time that may occur in the future, my rate is \$350/hour, with reimbursement of reasonable travel expenses as required.

### **Oath and Signature**

I declare under penalty of perjury under the laws of the United States of America that the foregoing is true and correct to the best of my knowledge, information, and belief.




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Kelly M. Socia, Ph.D.  
Associate Professor  
School of Criminology and Justice Studies  
University of Massachusetts, Lowell  
Kelly\_Socia@uml.edu

### **Supplemental Report Signed and Submitted: July 8, 2022**

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**For a Comprehensive C.V. of Kelly M. Socia, Ph.D. , and Expert Witness Qualifications and Publications (last 10 years), see Socia Amended Expert Report, above (Feb. 20, 2023).**